LETTERS TO THE EDITOR.

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Origin of Dun Horses.

IN discussing the colours and stripes of horses in "Animals and Plants under Domestication," Mr. Darwin says:—"I have endeavoured, but with poor success, to discover whether duns, which are so much more oftener striped than other coloured horses, are ever produced from the crossing of two horses, neither of which are duns. . . . One case, however, has fallen under my own observation of a foal from a black mare by a bay horse, which when fully grown was a dark yellow-dun and had a narrow but a plain spinal stripe."[1]

In a recent number (October 15) of the *Veterinary Record* Mr. J. B. Robertson gives the following instances of reversion to dun from the last eleven and first four volumes of the General Stud Book:—

(1) Bay-dun filly (1907), by Ash (chestnut), out of Unexpected (bay).

(2) Dun filly, Sarah Curran (1892), by Robert Emmett (bay or brown), out of Cellulites (black).

(3) Dun colt (1897), by Sir Frederick (bay), out of Lobelia (bay or brown).

(4) Light dun filly (1886), by Lord Gough (bay), out of Danseuse (brown).

(5) Dun or chestnut filly, Sancta (1884), by Exminster (bay), out of Halloween (chestnut).

(6) Dun filly (1763), by Young Cade (bay), out of Miss Thigh (grey).

(7) Dun colt (1730), by King George II.'s one-eyed grey Arabian, out of Young Kitty Burdett (bay).

(8) Dun filly (1829), by Lottery (brown), out of Octavia (bay).

Mr. Robertson also mentions (1) that a half-bred yellow-dun filly was obtained out of a liver-chestnut Welsh cob by a bay thoroughbred with a dorsal band—this filly "during early foalhood was profusely striped on the face, neck, and quarters"; and (2) that of 45 duns given in the tables included in his paper, 39 cannot be traced to an original dun ancestor. They sprang from the union of Silverlocks (chestnut) and the Godolphin Arabian (brown), "and hence afford incontrovertible evidence that a gametic line of duns—which in this case extended to four generations—may spring from parents neither of which are dun."

The University, Edinburgh. J. C. EWART.

Markings of Mars.

I HAVE recently returned by way of Tasmania from a series of visits to the chief observatories in the United States, which included a month's stay at the Lowell Observatory during the past opposition of Mars. This visit was made with the express object of testing by my own observation the reality of the data on which Dr. Lowell has based his speculations.

I find on my return that so much scepticism has been raised by the observations and arguments of M. Antoniadi and others that a record of my own experience may be of some value.

When I first looked at Mars at Flagstaff (September 27, 1909) I saw with great difficulty three streaks, presumably canals. The seeing was bad, and the general faintness of the planet's markings at that time is admitted by all. I continued to observe Mars on every possible night (which was nearly every night) until October 25, and as my eye became accustomed to the work I saw more and more. The canals were seen repeatedly better—this with the 24-inch refractor generally stopped down to about 18 inches. I found that with more than 20 inches the air was nearly always too unsteady, and with less than 15 inches too much separating power was lost. The canals were seen best with a power of 390 diameters.

Clearer they became each night until, on October 25, the seeing being the best I ever experienced, the canals came out with amazing clearness and steadiness, sharp and clean, like telegraph wires against the sky, the oases also being exquisitely defined. Whereas on previous nights the canals could be held only by short glimpses of perhaps half a second at a time, they were now steadily visible for three or four seconds together, when a short flicker would sweep over them, during the lucid intervals the limb also of the planet was perfectly steady, as I have never seen it before or since. Of the objective existence of these markings in the image at the focus of the telescope there could be no manner of doubt, and Lowell's representations of them are nearer the actual appearance than any I have seen, though even in his drawings the lines seem hardly fine enough. The effect produced on my mind by this remarkable definition, which lasted for upwards of one and a half hours (from about 8.30 until after 10 p.m.), was staggering and ineffaceable. Soon after ten the definition went to pieces.

It may be relevant to mention that a few evenings previously I had obtained a fair and convincing view of the canals with the 40-inch reflector (full aperture and a power of about 700), when they had appeared hazy and broader, but the image had been very unsteady, and only obtained in very short flashes; but nothing that I had hitherto seen had prepared me for the astonishing steadiness and *fineness* of the details visible on this superb night.

There is in my mind no sort of doubt that the revelation of this night was due both to the perfection of the instrument (which its maker long ago pronounced to be the best that the firm of Alvan Clark ever turned out) and the atmospheric conditions which are found at Flagstaff. With respect to these I would mention, as pointing to the freedom from water vapour, that I have seen the thermometer fall from more than 70° F. at 3 p.m. to below the freezing point at 3 a.m. without a trace of hoar-frost, and the general clearness of the air was such that I could see Uranus with the naked eye within 5° of the horizon, and could nearly every night count nine stars in the Pleiades and separate ϵ and 5 Lyræ.

The telescope also afforded on other nights ample evidence of the extraordinary clearness of the air. On many occasions both satellites of Mars, when not very near the limb, could be seen, without screening the planet, with 18 inches of aperture; and on one occasion with this aperture I picked up one of them unawares while looking for canals with *a yellow screen*. (N.B.—The importance of colour screens in rendering the canals visible does not seem to be sufficiently appreciated.)

In the face of all this positive evidence, and in the absence of any evidence that the observing conditions at Meudon, just outside Paris, ever approach these best conditions at Flagstaff, I find it impossible myself to attach any serious weight to the ingenious and plausible contentions of M. Antoniadi, which seem to have been much too hastily accepted in this country.

As to the deductions which Dr. Lowell has drawn from his observations I have nothing to say except that the startlingly artificial and geometrical appearance of the markings did *force* itself upon me.

JAMES H. WORTHINGTON.

Wycombe Court, High Wycombe, October 31.

November Meteors.

THE moon is full about the time when the Leonids become due in the present year, but that is no reason why these meteors should elude observation, for the Sickle has furnished some notable displays of shooting stars. With the moon in opposition in mid-November, as, for instance, in 1799 and 1867, though the coming apparition cannot be expected to vie as regards brilliancy with either of these historic events, yet in its way it may not prove unimportant nor be allowed to pass unobserved. Besides the Leonid epoch, there are also some other meteoric events that occur in November, of which the following particulars have been computed by the writer:—

Epoch, November 11, 9h. (G.M.T.), approximately second order of magnitude. Principal maximum,

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[1] "Animals and Plants under Domestication," vol. i., p. 62. (1872.)

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November 12, 5h. 45m.; secondary maximum, November 12, 9h.

Leonid epoch, November 17, 21h., twenty-eighth order of magnitude. Principal maximum, November 16, 13h. 45m.; secondary maxima, November 16, 13h. 20m. and 15h. 30m.

Epoch, November 19, 9h., eighth order of magnitude. Principal maximum, November 20, 15h. 15m.; secondary maxima, November 20, 6h. 30m. and 16h. 30m.

Epoch, November 19, 9h. 30m., fifth order of magnitude. Principal maximum, November 20, 14h. 30m.; secondary maxima, November 19, 20h. 30m., and November 21, 0h. 30m.

Epoch, November 23, 22h., approximately second order of magnitude. Principal maximum, November 21, 20h. 30m.; secondary maximum, November 22, 1h.

Epoch, November 28, 6h., approximately second order of magnitude. Principal maximum, November 30, 14h. 30m.; secondary maxima, November 30, 2h. 30m. and 11h. 30m.

It may be seen from the foregoing that there are four periods during the last three weeks of November that will probably be characterised by an unusual degree of meteoric activity, viz. November 12, 16, 20–21, and 30. The circumstance that the moon will be eclipsed in the night of November 16 may favour and stimulate Leonid observations, but the former phenomenon will perhaps have nearly ended before the latter may put in an appearance.

November 7. JOHN R. HENRY.

Early Burial Customs in Egypt.

It is suggested in Prof. Elliot Smith's letter (October 27, p. 529) that the burial customs in other countries influenced our observation of the burials in Egypt. On the contrary, the occasional practice of dismemberment in Egypt was a surprise to myself and to others; it is only gradually that the evidence for the wide distribution of such customs elsewhere has been brought forward as a parallel.

In place of all workers in Egypt finding "precisely the same state of affairs," many entire differences of custom are found in other material facts besides dismemberment, as thirty years' experience has proved.

The first principle for the archæologist to realise in Egypt is the great diversity of thought and custom which prevailed. With four totally incompatible beliefs about the future life, shown by diverse funeral customs throughout the history, it is quite natural that diversity should occur in the treatment of the body in the earlier ages. When the long-promised publication of Dr. Reisner on prehistoric Egypt is accessible, we shall be in a position to define some more localities where certain customs ruled. Discussion of these local variations before the fresh facts are published is premature.

W. M. FLINDERS PETRIE.

STRIPPED of all irrelevant considerations, the question at issue resolves itself into this, "Is there any real evidence to prove, or even to suggest, that the ancient Egyptians ever mutilated the bodies of their dead?"

In reply, I maintain that there is no evidence whatsoever capable of being twisted into the semblance of support to Prof. Flinders Petrie's contention.

Of all the multitudes of so-called "dissected burials" recorded by him, there is only one (see "Deshasheh," 1898) which carries conviction to those familiar with Egyptian conditions as a genuine case of secondary burial. Prof. Flinders Petrie says he has found two more cases this year. That may well be so. We found more than a score of such cases in Nubia.

But they are not evidence of deliberate mutilation of the body. They are all of them instances of some unintentional damage to the corpse—either by unskilled embalmers or by accident.

In reference to Prof. Flinders Petrie's closing remarks, I may state that by the time this letter is printed there will be published in Cairo Dr. Reisner's report (vol. i.) on the Archæological Survey of Nubia, containing his observations on prehistoric Egypt and Nubia.

G. ELLIOT SMITH.

Simulium and Pellagra.

THE interesting discovery by Dr. Louis Sambon that pellagra is due to a protozoal parasite conveyed by flies of the genus Simulium (NATURE, October 27) is, we may presume, merely the prelude to an energetic campaign of extermination directed against the insect.

It is well that medical men and sanitary officials should realise at the outset of such a campaign that the destruction of Simulium flies in any given area is an infinitely harder task than the destruction of mosquitoes. The larvæ of Simulium live in rapid streams, attached to submerged rocks and stones, and it is difficult to see how these streams can be drained dry if they are numerous in any particular district. Even if it were practicable to cover the surface of these streams with a film of oil, such a procedure would have no effect on the Simulium larvæ, for, unlike mosquito larvæ, the little creatures derive the oxygen necessary for their existence from the water bathing the gills situated at the anterior end of their bodies. In other words, the Simulium larva cannot be suffocated as can the mosquito larva.

Finally, it may be noted that the species of Simulium are very small flies, consequently to exclude them from houses wire gauze or muslin screens of extremely fine mesh must be employed. Such screens are bound to interfere seriously with the circulation of air in a house, and in a warm climate the discomfort entailed will be almost intolerable. R. SHELFORD.

Hope Department, Oxford University Museum.

The Cocos-Keeling Atoll.

DURING a very short visit to these islands some years ago I was taken across the lagoon in a light canoe, and when wading to land, about a quarter of a mile distant, over the rough surface of fresh coral branches, I suddenly crashed downwards for about 2 feet into a mass of rotten coral which spread over an irregular area some 20 or 30 yards across. I did not investigate this further, as a shark's fin appeared above the water off shore, but Mr. Ross informed me that a good deal of the coral in the lagoon had been "killed" at various times by sulphurous exhalations from below, and had become black and rotten in consequence. Mr. Ross (the owner of the island group) supposed that the wide and deep well-like holes and broad irregular patches of varying depth in the lagoon were due to this cause, which he compared to the sulphurous steam constantly roaring from the crater of the Gedeh and other mountains in Java.

If this comparison be correct, as it doubtless is, the Cocos ring is around the submerged summit of a volcanic cone which has not quite lost its solfataric activity. I have never seen it suggested that such poisonous exhalations coming into the still water confined within the atoll ring might account for the slower growth of the coral, and the deepening of the lagoon by the degradation of the coral branches where the polyps had been suddenly poisoned. It is, however, possible that some such influence may cooperate to prevent the coral flourishing as rapidly as it does outside the ring in the boisterous wash of the fresher waves that are constantly stirred by the trades.

I have not yet read Mr. Wood-Jones's book, but it was the decided opinion of Mr. Ross, founded upon boat navigation, that the lagoon was shallowing, because, as he thought, the submerged summit was slowly rising. If this be so something more than slower growth is necessary to account for the continued existence of the lagoon, since, however slow the growth, it must ultimately in a rising area bring the summit up at least to water-level; but if there is this kind of active degradation, neither slow upheaval nor slower growth could prevail against such rapid destruction, and a comparatively deep atoll with irregular bottom contours would result.

Waterstock, Oxon, October 31. E. C. SPICER.

IT would be ungenerous, after the frank admissions of inaccuracy on the part of the reviewer (NATURE, October 27), to criticise the substance of his review in any more detail; but it is necessary to make some reply to his assertions concerning the development of atolls.

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